TELECOPIER COVER SHEET

DATE: 4-17-89

TO: Dick Garwin

IBM

TELECOPIER NO. 914-945-2141

VERIFICATION NO.

FROM: Harold Furth

PRINCETON UNIVERSITY
PLASMA PHYSICS LABORATORY
JAMES FORRESTAL CAMPUS
PRINCETON, NEW JERSEY 08543

TELEPHONE NO. 609-243-3555

DEPARTMENT:

THIS TRANSMITTAL CONSISTS OF 3 PAGE(S) EXCLUDING THE COVER SHEET.

SPECIAL INSTRUCTIONS:

Dr. Garwin
- give to him
at lunch

cc: Jean 10:40
Barbara Stack 4/17/89

Telecopier No. 609-243-2749
FTS-340-2749

PROGRESS TOWARDS THE ACHIEVEMENT OF PRACTICAL FUSION POWER

HAROLD P. FURTH
PRINCETON UNIVERSITY, PLASMA PHYSICS LABORATORY
PRINCETON, NEW JERSEY 08543

PRESENTED AT
AMERICAN CHEMICAL SOCIETY NATIONAL MEETING

DALLAS, TEXAS
APRIL 12, 1989

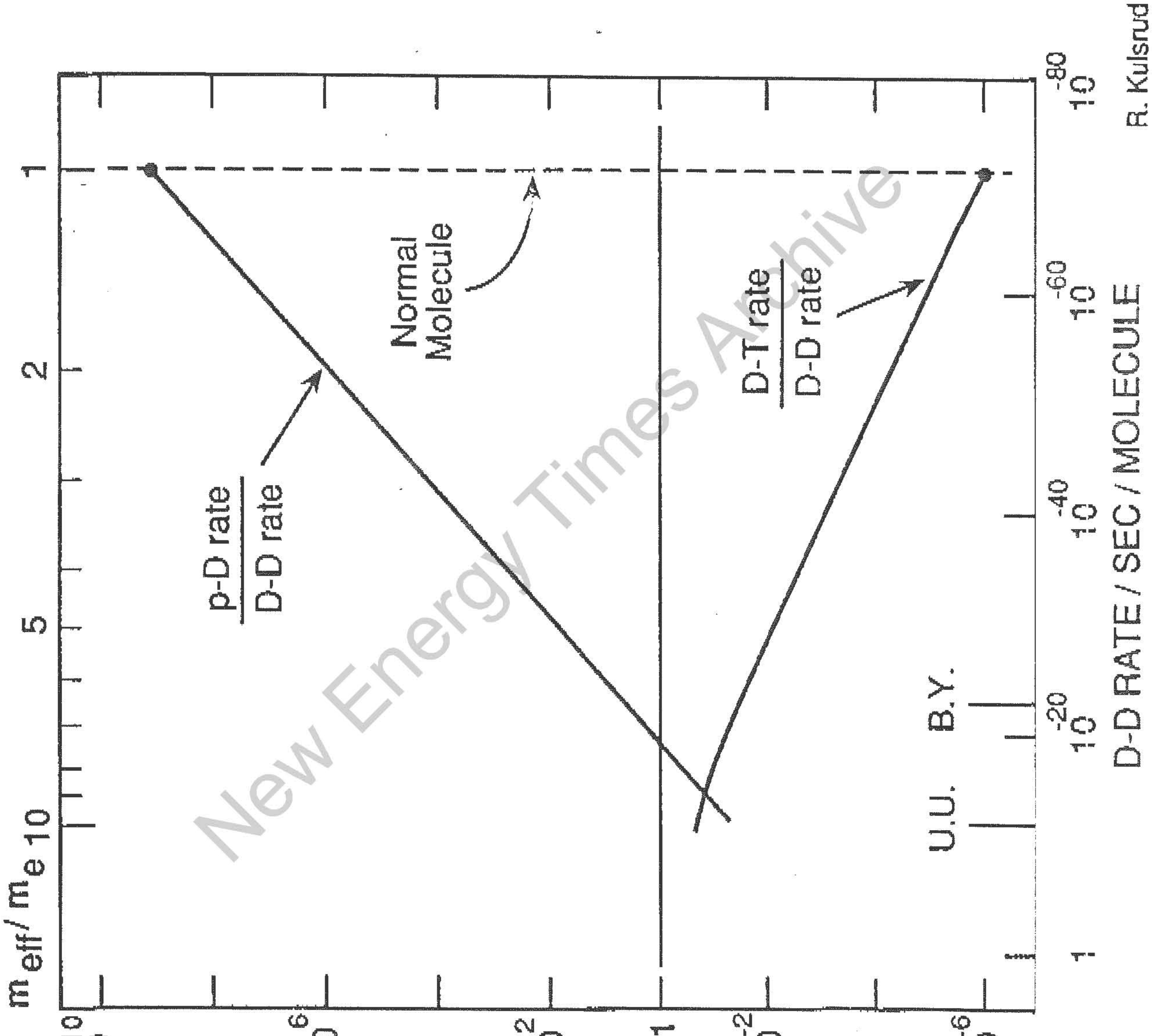
Dick:

Here are two of my viewgraphs from Dallas. If you disagree with the graph, please call Russell Kulsrud (609) 243-2613, who calculated it.

H.

#89T0062

RELATIVE RATES OF FUSION REACTIONS



R. Kulsrud

PROPOSAL FOR INVESTIGATING LOW RATES OF "COLD FUSION" EVENTS

- INVESTIGATE P-D, RATHER THAN D-D REACTIONS (E.G., USE A 50/50 LIGHT/HEAVY WATER COCKTAIL).

- LOGIC:

  1. THE P-D SOURCE BECOMES STRONGER THAN THE D-D SOURCE AT LOW REACTION RATES, AND THE 5.5-MEV GAMMA RAY OF THE $p + D \rightarrow {}^3He + \gamma$ REACTION IS ABOUT AS EASY TO IDENTIFY AS THE 2.45 MEV NEUTRON OF THE $D + D \rightarrow {}^3He + n$ REACTION.

  2. MEASURING THE APPROXIMATE RATIO OF P-D TO D-D REACTIONS PROVIDES A DIRECT TEST FOR "COLD-FUSION" TUNNELLING.

Richard L. Garwin
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April 17, 1989
(Via FAX to 011 (44-1) 836-9934)

Dr. Laura J. Garwin
Physical Sciences Editor
Nature
Macmillan Journals Ltd.
4 Little Essex Street
London WC2R 3LF
ENGLAND

Dear Laura,
&RET

Also Adjunct Professor of Physics at Columbia University
(Views not necessarily those of IBM or Columbia)

For Dr. Roland Pease, NEWS AND VIEWS. AND Dr. Laura Garwin, Physical Sciences Editor, Nature.
FAX 011-441- 836-9934

April 16, 1989 23:27 New York time.

Dear Drs. Pease and Garwin:

This is written on the assumption that I have 1200 words because the actual paper(s) will be published in the following issue. However, I have not actually counted the words, and I apologize for sending this precursor without being able to format it visually for FAX transmission. It will be double spaced. By 0830 EDT, I will send the formatted version, but you may want to work on this sooner and to send me lined-out and marked up copy to my FAX at IBM:
for Richard L. Garwin (914) 945-2141.

If I have only 900 words, please hint or direct modifications. Let me just explain my whereabouts on Monday. I will be at Yorktown, at an IBM Science Advisory Committee meeting all day. However, I am very much available, both to read and to write. The easiest way to communicate with me is by FAX, but if you want to talk with me, call me at my regular number (914) 945-2555 and ask for me to return the call within a few minutes.

After my meeting starts at 0830, I may find a FAX closer to the conference room. If so, I'll let you know. Also, I hope to find first thing a FAX with the names and addresses of those at the Erice Forum, so that I can clarify some "??" in the Meeting Report.

Sorry to make you puzzle out superscripts: ^{xx} and subscripts, etc.

R.L. Garwin ***********

An International Forum on Cold Nuclear Fusion*

---

* April 12, 1989, at "Ettore Majorana" Center for Scientific Culture, Erice, Sicily, convened by Antonino Zichichi, Director.

---

began with presentations by M. Fleischmann[1]

---

1. Fleischmann, M...., J. Electroanalytical Chemistry, ...

---

of the work at the University of Utah, and by S.E. Jones and J.B. Szirr[2]

---

2. Jones, S.E.,.... ***03/23/89 submitted to NATURE.. How do I refer to this?***

---

of their work at Brigham Young University, Provo, Utah, on room temperature nuclear fusion of deuterium-- cold nuclear fusion. A full day of presentations and discussion brought no consensus on the results, no credible theory to explain them, but some suggestions as to where to look for explanation or confirmation.

FAMILIAR REACTIONS IN UNEXPECTED CIRCUMSTANCES?

Jones, et al, electrolyze[2] heavy water (99.5% D_2O) in a pH 3 solution of a witch's brew of salts (including Li and Pd), with rough Ti chunks ~~or~~ Pd foil as cathode, driving deuterium into the metal with a supply voltage of 3-25 V at cell currents of 10-500 mA. A gold foil was used as anode in each 20-ml cell. A counter designed to detect and identify fast neutrons indicates a total of 170 +/- 23 counts with a pulse-height spectrum consistent with that expected for the 2.45 MeV neutron of the well-known fusion reaction:

$$d + d = {}^3He\ (0.82\ MeV) + n\ (2.45\ MeV)\quad (i)$$

J.B. Czirr described the neutron detector in detail, which detects a thermalized neutron by the light flash in ^6Li-doped glass and determines the neutron energy by the overall fast light pulse emitted in the preceding 20 microseconds from a liquid scintillator in which the proton recoils occur as the neutron thermalizes. The counting rate is but 2 per hour in the relevant region of pulse height.

Analogous experiments[1] used a strongly alkaline solution of 0.1 M LiOD in heavy water to drive deuterons into Pd rod cathodes (cast and machined) under the influence of cell currents up to 800 mA and voltages typically of 12 V. Reaction (i) is to be detected by the 2.22 MeV gamma ray that results from capture of the 2.45 MeV neutron (after thermalization) ~~on~~ by a proton of the surrounding water bath:

$$p + n = d + \gamma \text{ (2.224 MeV)} \qquad \text{(ii)}$$

A very narrow peak at 2.2 MeV containing some 3000 γ-ray counts is shown for an NaI scintillation detector close to the electrolytic cells, in comparison with the "level spectrum" in a similar detector 5 m or 10 m away. Unfortunately, the full pulse-height spectrum is not shown, so one can't verify the presence of the annihilation radiation "escape peaks" that would lend more confidence to the neutron origin of these counts. Furthermore, no evidence connects the counts with current applied to the cell, and there have been no runs with ordinary water as a control.

The scientific and patent literature for the last 60 years have occasional claims of nuclear fusion catalyzed by Pd, but there have previously been no credible reports of neutrons from metal deuterides for reasons thought to be well understood: the Coulomb barrier to close approach by nuclei of charges Z_1e and Z_2e amounts to $(Z_1e)x(Z_2e)/r$, or about 600 keV for $Z_1=Z_2=1$, reducing the quantum mechanical tunneling probability to a very small value for deuterons in a D_2 molecule to approach to with the range of nuclear forces. New calculations were presented to the Forum[3] (Koonin, Caltech) of the fusion reactions per second per deuteron $\lambda_{f\text{-}dd}$ bound in a deuterium molecule by "electrons" of normal charge but mass m* instead of m_e:

/S, E,

Table 1. Base-10 logarithms of fusion rates per deuteron per second.

m^*/m_e	1	2	5	10	(200)
$\lambda_{f\text{-}dd}$	-63.5	-40.4	-19.8	-09.1	(+6)
$\lambda_{f\text{-}pd}$	-55.0	-36.0	-19.0	-10.4	

Other participants agreed with these results, which correct an error in some previous calculations and use a more accurate molecular potential.

---

[3] Koonin, S.E., and Nauenberg, M., (submitted to Nature 7 April, 1989)

An important experimental point is provided by the $m^*=200$ case, in which a negative muon binds two deuterons as a molecular ion 200 times smaller in dimension than the normal molecular ion, with $\lambda_{f\text{-}dd}$ of some $10^6\ s^{-1}$ as predicted[3] and observed[4]. ? The results of Table 1 show the strikingly easier penetration of the barrier by the proton (because of the "reduced mass" = μ $M_1M_2/(M_1+M_2)$ in the barrier penetration factor $B = e^{-\int k(r)dr}$, where $k(r)=[2\mu(V(r)-\varepsilon)]^{1/2}$ and the integral runs from 0 to the classical turning point r_0. In the region of the claimed neutron production rate[2], $\lambda_{f\text{-}dd}=10^{-20}$, so that $m^*/m_e=5$ from Table 1. A similar value is needed to explain the γ-ray counts[1].

Although quasi particles of high effective mass are well known in metals, their m^* relates to density of states vs. energy in the band structure of the lattice; a quasi particle of $m^*=5$ is not capable of binding two deuterons to a density $5^3=125$ times that of molecular hydrogen, or of allowing the nuclei to approach one another to a distance 5 times smaller than the 0.74 Å ~~&Angstrom.~~ internuclear separation in D_2 -- 0.15 Å ~~&Angstrom.~~, at which the repulsive potential amounts to some 95 eV.

---

[3] F.C. Frank, Nature, V. 160, 525 (1947). [4] L.W. Alvarez, et al., Phys. Rev., V. 105, 1127 (1957).

It was suggested therefore to look for dynamic effects to augment the equilibrium tunneling-- phonon-assisted tunneling (Koonin), or coherent acceleration (Ponomarev, Karchatov Institute) ~~USSR)~~ in which traveling electron density waves could trap deuterons and accelerate them to the same velocity as the waves; the deuteron kinetic energy would then be some 3700 times that of an electron of the same velocity and would greatly enhance the tunneling through the barrier. Alternatively, a solution could be sought in "high-T_c superconductivity or other miracle of solid-state physics."

Several experimental groups at the Forum presented results showing no neutrons or gamma rays generated in replication of (1) or (2). Electrolyzing 1-mm by 10-cm Pd rods for 10 days gave neutron yields below 0.6 s^{-1}-cm^{-3} (M.M. Broer, AT & T Bell Labs), (less than 10^{-3} ~~that~~ of that reported in similar circumstances[1]) and an upper limit of 10^{-3} s^{-1}-cm^{-3}

was set for the detection of t or ~~n~~ p from the d+d reaction (J. ~~E.~~ F. Ziegler, IBM). Experiments were reported (F. Celani, Frascati) with "some increase in neutron signal at the beginning of each experiment for about 5 minutes, but indistinguishable from background after 20 minutes." Experiments will be transferred to the great underground laboratory at Gran Sasso; perhaps also those who claim the ability to produce neutrons will be hospitable to those more adept at detecting than at producing them in this way.

NON L'AVREI GIANMAI CREDUTO...

MA FARO QUELCHE POTRO...

("I would never have believed this, but I'll see what I can do," Don Giovanni, quoted by L. Maiani~~??)~~, U. of Rome)

Commenting on the store St., the evening at the dinner party.)

NATURE should decide if it wants to use this quotation. If so, you or I will have to get the precise wording

This, of course, refers to the heat generation claim'd ~~claimed~~[1] of some 10 W per cc for 100 hours or more, as well as destructive releases of heat that fuse and vaporize Pd and destroy the cell. Although the latter is likely to result from the electrochemical creation of high explosive by stuffing hydrogen into high-energy sites in Pd (analogous to the Wigner energy in neutron-irradiated graphite), no such explanation can account for the 4 MJ/cc or 600 eV per atom to which such continuous energy release would correspond, if indeed there is such excess heat flow to the surrounding water bath, in excess of that represented by the product of current and voltage applied to the cell (typically 0.8 A at 12 V). Stored energy could be at most 3 eV or so in any chemical reaction (~~in comparison~~ compared with the 600 eV), so it is of the utmost importance to inquire into the details of this measurement; unfortunately, details are lacking.

The "excess enthalpy generation" is measured calorimetrically[1] by a "calibrated thermistor" as a ΔT between the (sometimes stirred) contents of the electrolytic cell and a surrounding thermostated water bath, in comparison with the ΔT measured for a resistance heater in the cell; the thermal impedance is that posed by the dewar flask in which each experiment is conducted. For rods of 1 mm, 2 mm, 4 mm diameter, an excess heat rate is found[1] that depends strongly on the current density, amounting to 8-20 W/cc at 0.5 A/cm^2, ~~and that the~~ which excess heat persists during the operation of the cell for hundreds of hours, even though the surface of the Pd rod "blockens" ~~darkens~~.

I have seen insufficient evidence to believe that there is "excess heat," since the ΔT is measured between the bath and a ~~the~~ thermistor in the electrolytic cell, rather than along a fixed conductive link between cell and bath. ~~Specifically,~~ If there are significant temperature gradients within the cell because of imperfect stirring or local recombination of hydrogen and oxygen gas, the thermistor temperature will not be the temperature of the inside wall of the flask, resulting in very substantial errors in inferred heat generation.

Even more striking than a heat-producing fusion reaction at a rate some 6×10^{12} cm^{-3} s^{-1} is the simultaneous claim ~~of detection~~ of only 4×10^{3} neutrons produced cm^{-3} s^{-1}; fewer than 10^{-9} of the reactions are supposed to produce a neutron. How can this happen? A coherent superposition of isotopic spin states for d+d could cancel the neutron-producing reaction and reinforce the t+p branch (D. Wilkinson, Suggest) ~~??~~), but would not eliminate the usual isospin-zero reaction; it would thus change the neutron branching ratio only if the barrier penetration were greatly facilitated. This effect can be estimated, and I judge the effect is only a few per cent rather than a factor 10^9.

Suggestions of radiationless de-excitation of the ^{4}He intermediate state formed by d+d thus far fail in two regards: first, the lack of a mechanism, and second, because such a mechanism would add a channel to the usual particle decay of ^{4}He rather than suppress the usual channel. Thus such a mechanism would need to be 10^9 times faster than the usual particle channels that themselves occur in nuclear transit times-- a totally new phenomenon. Finally, the needed mechanism must not have shown itself in the measurement of the cross sections-- some of which were done in gas cells, but some, at times, in metal hydrides.

"SOMEBODY IS GOING TO EAT HIS HAT" (L. Maiani~~, ??~~)

"WE ARE ALSO HUMAN, AND NEED MIRACLES, AND HOPE THEY EXIST" (Ponomarev~~, USSR~~).

A few neutrons each second (or a few thousand) from an electrolytic cell may be cold nuclear fusion, or may be due to an "arcs and sparks" origin. Within the next few weeks, experiments will surely show whether cold nuclear fusion is taking place; if so, it will teach us much besides humility and may indeed provide insight into significant geophysical puzzles[2]. Large heat release from fusion at room temperature[1] would be a multi-dimensional revolution; I bet against its confirmation.

Sincerely yours,

Richard L. Garwin

RLG:rlg:107%LJG:041789.LJG

7.1

Consensus on cold fusion still elusive

Accounts of the cold fusion experiments at the University of Utah and Brigham Young University were presented ~~last week~~ at a meeting at the Ettore Majorana Centre for Scientific Culture, but many questions remain unanswered.

~~April~~ 12

Erice, Sicily

After a full day of presentations and discussion of the recent claims of cold nuclear fusion, there was no consensus on the results, no credible theory to explain them, but some suggestions as to where to look for an explanation ~~or confirmation~~. The work of the Utah group[1] was presented at the meeting last week by M. Fleischmann, that of the Brigham Young group[2] by S.E.Jones and J.B.Szirr.

U. of

The details of the experiments are important in any comparison or assessent. Jones and his colleagues electrolyze heavy water (D$_2$O in a solution ~~at pH 3 of~~ a witches' brew of ~~salts~~ (including Li and Pd) with rough Ti ~~or Pd~~ chunks as cathodes, driving deuterium into the metal with a voltage of 3-25 V and cell currents of 10-500 mA. In each 20-ml cell, the anode is gold foil. A counter designed to detect and identify fast neutrons indicates a total of 170 ± 23 counts with a pulse-height spectrum consistent with that expected for the 2.45 MeV neutron of the well-known fusion reaction:

Pd foil or

$$d \cancel{\pm} d = {}^3\mathrm{He}\ (0.82\ \mathrm{MeV}) \pm n\ (2.45\ \mathrm{MeV}) \qquad (1)$$

+

+

~~Last week,~~ J.B. Czirr described the neutron detector in detail. It detects a thermalized neutron by the light flash in ^{6}Li-doped glass. The neutron energy is determined by the overall fast light-pulse caused by protons recoiling in a liquid scintillator as the neutrons are thermalized. The counting rate is but 2 ~~an~~ hour in the relevant region of pulse height.

per

The analogous experiments[1] by Fleishmann ~~and~~ Pons use a strongly alkaline solution of 0.1 M LiOD in heavy water and drive deuterons into Pd rod cathodes (cast and machined) under the influence of cell currents up to 800 mA and voltages typically of 12 V. The plan is to detect reaction (1) by the 2.22 MeV γ-ray resulting from capture of the 2.45 MeV neutron (after thermalization) ~~of~~ a proton of the surrounding water bath:

, and Hawkins

by

$$p + n = d + \gamma\ (2.224\ \mathrm{MeV}) \qquad (2)$$

041789.RLG

The authors describe a very narrow peak at 2.2 MeV containing some 3000 γ-ray counts for an NaI scintillation detector close to the electrolytic cells, in comparison with the "level spectrum" in a similar detector 5 m or 10 m away. Unfortunately, the full pulse-height spectrum is not shown, so that it is not possible to verify the presence of the annihilation radiation "escape peaks" that would lend more confidence to the origin of these counts in the neutron-proton interaction (2). Furthermore, no evidence has been given which connects the counts with current applied to the cell, and there have been no runs with ordinary water as a control.

In the scientific and patent literature over the past 60 years, there have been occasional claims of nuclear fusion catalysed by palladium, but there have previously been no credible reports of neutrons from metal deuterides for reasons thought to be well understood.

Briefly, these are that the Coulomb barrier to close approach by nuclei of charges Z_1e and Z_2e amounts to $(Z_1e)\times(Z_2e)/r$, where r is the nuclear diameter. This amounts to about 600 keV when $Z_1 = Z_2 = 1$, reducing to a very small value the probability that, by quantum mechanical tunnelling, the deuterons in a D_2 molecule will approach within the range of nuclear forces.

Even so, the calculation of the rate at which deuterium nuclei in a molecule will undergo fusion is important as a yardstick for assessing the rates reported in the recent experiments. At last week's forum, new calculations were presented[1] by S.E.Koonin (Santa Barbara) of the number of fusion reactions per deuteron bound in a deuterium molecule by "electrons" of normal charge but mass m* instead of m_e. If the logarithm (base 10) of the number of fusions per deuteron per second is λ (with a subscript to indicate the fusion mode), the results come out as follows:

delete (200) (6) column

INSERT TABLE COPY HERE

Others at the meeting agreed with these results, which correct an error in some previous calculations and use a more accurate molecular potential. An important experimental point is provided by the case in which m* = ~~200, corresponding to~~ 207 that in which a negative muon binds two deuterons as a molecular ion ~~200~~ 207 times smaller in dimensions than the normal molecular ion, with λ_{dd} of some 10[illegible] s^{-1} 10⁹, as predicted[3] and observed[4].

These results also show the strikingly wide easier penetration of ~~the~~ barriers by the proton (~~for wide barriers~~). The explanation lies in the sensitivity of the chance that the Coulomb barrier will be penetrated by quantum mechanical tunnelling to the "reduced mass" μ of the two nuclei, which is $M_1M_2/(M_1 + M_2)$. Numerically, the barrier penetration factor is e [illegible], where the integral runs from zero to the classical turning point r_0 and the function $k(r)$ is $[2\mu(V(r) - \varepsilon)]^{1/2}$.

$$e^{-2\int k(r)\,dr}$$

The rate of neutron production claimed by ~~Fleischmann and Pons~~ Jones, et al, is $\lambda_{dd} = 10^{20}$, which would require that m*/m_e were

(416)

equal to 5, according to the figures in the table. A similar value of the effective mass is needed to explain the γ-ray counts reported by ~~Jones et al.~~ Fleischmann and Pons.

Although quasi-particles of high effective mass are well known in metals, the value of m^* relates to the relationship between the density of states and the energy in the band structure of the lattice. Thus a quasi-particle of effective mass 5 is not capable of binding two deuterons to a density 5^3, or 125, times that of molecular hydrogen, or of allowing the nuclei to approach one another to a distance 5 times smaller than the 0.74 Å internuclear separation in the D_2 molecule; ~~but~~ at this distance, 0.15 Å, the repulsive potential amounts to some 95 eV.

That is why it was argued at the forum last week that one should look for dynamic effects to augment the equilibrium tunnelling — phonon-assisted tunnelling (Koonin) or coherent acceleration (Ponomarev, USSR) in which travelling electron density waves may trap deuterons and accelerate them to the same velocity as the waves. The deuteron kinetic energy would then be some 3,700 times that of an electron of the same velocity and would greatly enhance the chance of penetrating through the barrier by quantum tunnelling. Alternatively, a solution could be sought in "high-T_c superconductivity or other miracle of solid-state physics".

Several experimental groups at the forum presented results showing no neutrons or γ-rays generated in replication of the experiments which have been described [1,2]. Pd Electrolyzing 1-mm by 10-cm rods for 10 days gave neutron yields below 0.6 $s^{-1}cm^{-3}$ (M.M. Broer, AT&T Bell Laboratories) or less than 10^{-3} of those reported by the Brigham Young group in similar circumstances. ~~This sets~~ an upper limit of $10^{-3}s^{-1}cm^{-3}$ for the detection of *t* or *p* from the *d+d reaction* (J.E. Ziegler, IBM) reported

F

Experiments were reported (Celani, Frascati) with "some increase in neutron signal at the beginning of each experiment for about 5 minutes, but indistinguishable from background after 20 minutes". Experiments will be transferred to the great underground laboratory at Gran Sasso; perhaps also those who claim the ability to produce neutrons will be hospitable to those more adept at detecting than at producing them in this way.

Non l'avrei giammai creduto...
Ma farò quelche potro... } bold

("I would never have believed this, but I'll see what I can do," Mozart's Don Giovanni, quoted by L. Maiani)

This, of course, refers to the heat generation claimed[1] of some 10 W per cm^{3}cc for 100 hours or more, as well as destructive releases of heat that fuse and vaporize Pd and destroy the cell. The most likely explanation of such violent happenings is that they are likely to be the result of the electrochemical creation of high explosive by stuffing hydrogen into high-energy sites in Pd (analogous to the Wigner energy in neutron-irradiated graphite). But no such explanation can account for the 4 MJ/cm^3 (or 600 eV per atom) to which continuous energy release at such a rate would correspond.

indeed
any

One issue to ~~be~~ checked is whether there is ~~indeed~~ such excess heat flow to the surrounding water bath, in excess of that represented by the product of current and voltage applied to the cell (typically 0.8 A at 12 V). Stored energy could be at most 3 eV or so in any chemical reaction, so it is of the utmost importance to inquire into the details of this measurement; unfortunately, details are lacking.

The "excess enthalp~~hy~~ generation" is measured calorimetrically[1] by a "calibrated thermistor" as a ΔT between the ~~(sometimes stirred)~~ contents of the electrolytic cell and a surrounding thermostated water bath, in comparison with the ΔT measured for a resistance heater in the cell; the thermal impedance is that posed by the dewar flask in which each experiment is conducted. For rods of 1-mm, 2-mm and 4-mm diameter, an excess heat rate is found[1] that depends strongly on the current density, amounting to 8-20 W/cm^3 at 0.5 A/cm^2, which excess heat persists during the operation of the cell for hundreds of hours, even though the surface of the Pd rod blackens.

I have seen insufficient evidence to believe that there is "excess heat", since the ΔT is measured between the bath and the thermistor in the electrolytic cell itself, rather than along a fixed conductive link between cell and bath. ~~Specifically,~~ if there are significant temperature gradients within the cell because of imperfect stirring or local recombination of hydrogen and oxygen gas, the thermistor temperature will not be the temperature of the inside wall of the flask, resulting in very substantial errors in inferred heat generation.

Even more striking than a heat-producing fusion reaction at a rate some 6×10^{13} cm^{-3} s^{-1} is the accompanying claim to have detected the production of only 4×10^3 neutrons per cm^3 per scond. This means that fewer than 1 in 10^9 of the reactions are supposed to produce a neutron.

6/6

How can this happen? A coherent superposition of isotopic spin states for $d+d$ could cancel out the neutron-producing reaction and reinforce the $t+p$ branch (D. Wilkinson, Sussex), but would not eliminate the usual isospin-zero reaction. It would thus change the neutron branching-ratio only if the barrier penetration were greatly facilitated. This effect can be estimated, and I judge the effect is only a few per cent rather than a factor 10^9.

Suggestions of radiationless deexcitation of the ^{4}He intermediate state formed by $d+d$ thus far fail in two regards: first, because the lack of a mechanism and, second, such a mechanism would add a channel to the usual particle decay of ^{4}He rather than suppress the usual channel. Thus such a mechanism would need to be 10^9 times faster than the usual particle channels that themselves occur in nuclear transit times — a totally new phenomenon. Finally, the needed mechanism must not have shown itself in the measurement of the cross sections — some of which were done in gas cells, but some, at times, in metal hydrides.

Somebody is going to have to eat his hat. (L. Maiani)

We are also human, and need miracles, and hope they exist (L. Ponomarev, Moscow)

A few neutrons each second (or a few thousand) from an electrolytic cell may be cold nuclear fusion or may have an "arcs and sparks" origin. Within the next few weeks, experiments will surely show whether cold nuclear fusion is taking place; if so, it will teach us much besides humility and may indeed provide insight into significant geophysical puzzles^2. Large heat release from fusion at room temperature^1 would be a multi-dimensional revolution. I bet against its confirmation.

Richard L. Garwin

«ETTORE MAJORANA» CENTRE FOR SCIENTIFIC CULTURE

Attention: Dr. Richard GARWIN

Dear Dr. Garwin,

Here is the information that you requested.

Best regards,
Jerry Pilarski

Prof. Francesco CELANI
LNF-INFN
Via Enrico Fermi
00044 FRASCATI, Italy
Tel: 39 (6) 9403342

Prof. Luciano MAIANI
Dipartimento di Fisica
Università La Sapienza
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Sir Denys WILKINSON FRS
Past Vice Chancellor
Department of Physics
University of Sussex
FALMER BN20 0BA, UK

Dr Richard Garwin
from John Maddox
17 April 1989
Here is a proof (1+5 pages)
of the News and Views leader.
Pls call me on 01-240 2015.
If I'm not there, try
01-240 3858.
This proof has not been read.

Attention: Dr. Richard GARWIN

Dear Dr. Garwin,

Here is the information that you requested.

Best regards,
Jerry Pilarski

Prof. Francesco CELANI
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041789..JP

R.L. GARWIN-

APR 89 10. 74

TELECOPIER COVER SHEET

DATE: 4-17-89

TO: Dick Garwin

IBM

TELECOPIER NO. 914-945-2141

VERIFICATION NO.

FROM: Harold Furth

PRINCETON UNIVERSITY
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JAMES FORRESTAL CAMPUS
PRINCETON, NEW JERSEY 08543

TELEPHONE NO. 609-243-3555

DEPARTMENT:

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PROGRESS TOWARDS THE ACHIEVEMENT OF PRACTICAL FUSION POWER

HAROLD P. FURTH
PRINCETON UNIVERSITY, PLASMA PHYSICS LABORATORY
PRINCETON, NEW JERSEY 08543

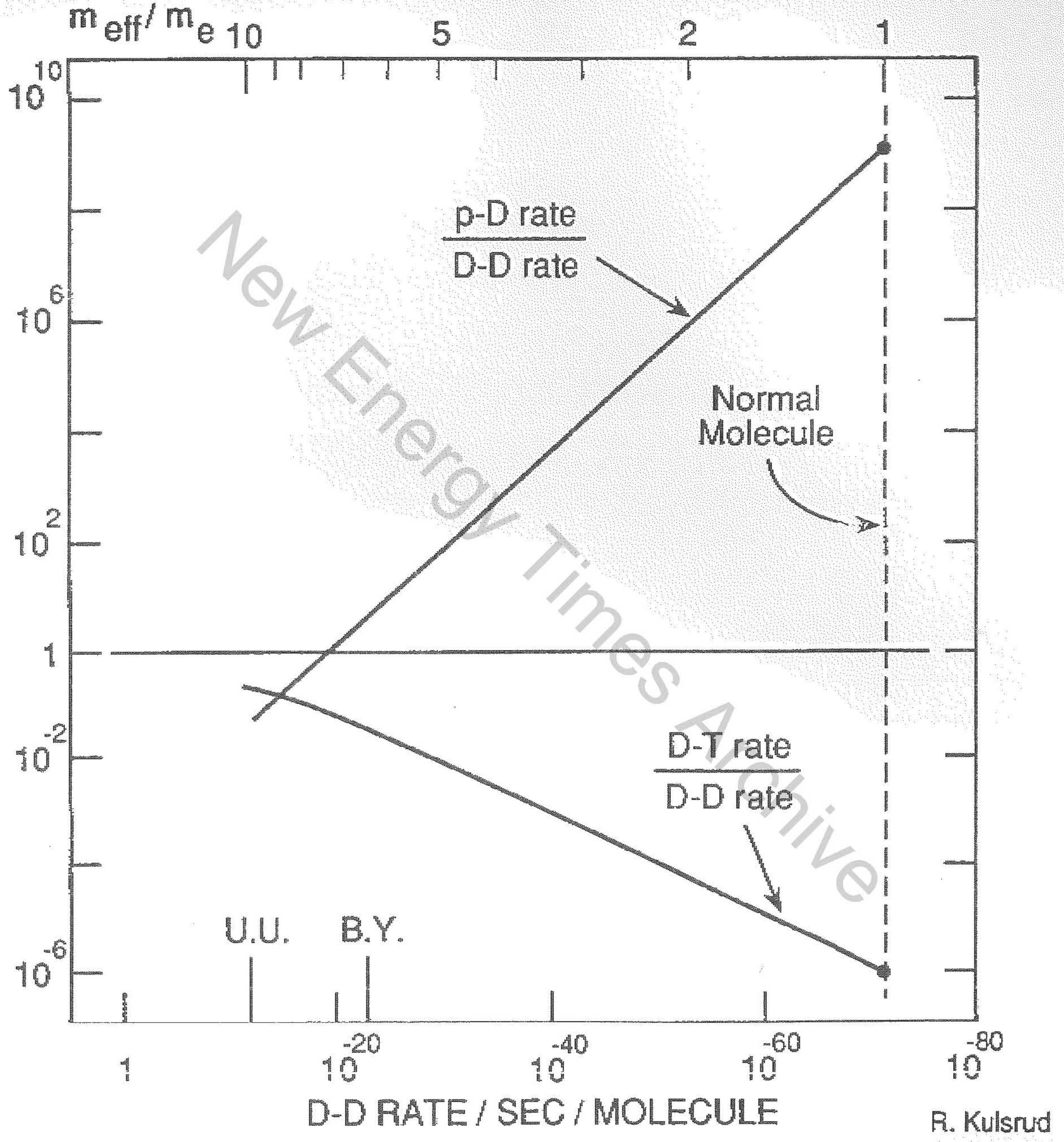
PRESENTED AT
AMERICAN CHEMICAL SOCIETY NATIONAL MEETING

DALLAS, TEXAS
APRIL 12, 1989

Dick:
Here are two of my viewgraphs from Dallas. If you disagree with the graph, please call Russell Kulsrud (609) 243-2613, who calculated it.
H.

#89T0062

RELATIVE RATES OF FUSION REACTIONS



R. Kulsrud

PROPOSAL FOR INVESTIGATING LOW RATES OF "COLD FUSION" EVENTS

- INVESTIGATE P-D, RATHER THAN D-D REACTIONS (E.G., USE A 50/50 LIGHT/HEAVY WATER COCKTAIL).

- LOGIC:

  1. THE P-D SOURCE BECOMES STRONGER THAN THE D-D SOURCE AT LOW REACTION RATES, AND THE 5.5-MEV GAMMA RAY OF THE $p + D \rightarrow {}^3He + \gamma$ REACTION IS ABOUT AS EASY TO IDENTIFY AS THE 2.45 MEV NEUTRON OF THE $D + D \rightarrow {}^3He + n$ REACTION.

  2. MEASURING THE APPROXIMATE RATIO OF P-D TO D-D REACTIONS PROVIDES A DIRECT TEST FOR "COLD-FUSION" TUNNELLING.

Telephone 01-836 6633
Telex 262024

• TELEFAX MESSAGE •

p3,2(

FROM . LAURA GARWIN . .

TELEPHONE 01 836 6633 Ext. 2230.

FAX 01 836 9934 or 01 379 4204

TELEX 262024 MACMIL G

TO . DR. R. L. GARWIN
(in SAC meeting).

SUBJECT . Cold fusion - supplementary material

PAGE ONE of . 7 . . PAGES

DATE . 17 April 1989 . .

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THE COLD FUSION FAMILY

James S. Cohen
Theoretical Division
Los Alamos National Laboratory
Los Alamos, New Mexico 87545, USA

and

John D. Davies
School of Physics and Space Research
University of Birmingham
Birmingham B15 2TT, UK

Two reports in this issue claim fusion from electrolysis of D_2O at a palladium or titanium electrode. One[1] sees 2.5 MeV neutrons, presumably from dd fusion, 5 standard deviations above background. The other[2] has considerable production of heat that, if the same fusion reaction, would appear to be deficient in neutrons by a factor of $\sim 10^9$ even though the number is $\sim 10^4$ times greater than the neutrons in Ref. 1. To constrain speculation this note gives information obtained (mainly) from the established form of cold fusion—that catalysed by muons.[3]

The nuclear processes of concern are

$$d + d \longrightarrow \begin{cases} t(1.01\ \text{MeV}) + p(3.02\ \text{MeV}) & (42\%\ \text{in}\ \mu\text{CF}) \quad (1a) \\ {}^3\text{He}(0.80\ \text{MeV}) + n(2.45\ \text{MeV}) & (58\%\ \text{in}\ \mu\text{CF}) \quad (1b) \end{cases}$$

$$d + t \longrightarrow {}^4\text{He}(3.5\ \text{MeV}) + n(14.1\ \text{MeV}) \qquad (2)$$

The nuclei are held apart by their Coulomb repulsion. A $dt\mu$ molecular ion is $\sim m_\mu/m_e$ (=207) times smaller than dte (i.e., DT^+). This reduces the tail of the Coulomb barrier so that fusion[4] occurs in 8×10^{-13} s for $dt\mu$ and 2×10^{-9} s for $dd\mu$. However, it is important to note that this time difference arises in part because muon–catalysed fusion (μCF) of dd occurs in a state with an angular momentum barrier (both muonic molecules are formed via very *unusual* resonant states with $J=1$, but $\Delta J=1$ transitions of $dd\mu$ are not allowed because of its nuclear symmetry). In nuclear scattering or in bound states prepared by "ordinary" means, the fusing dd will normally have $J=0$; henceforth only $J=0$ states will be considered. In order to understand cold fusion better, it is helpful to examine its relation to fusion in ordinary nuclear scattering.

The fusion cross sections are usually written as the product of two factors: one representing the strength of the interaction (astrophysical S function) and a second representing the penetrability of the Coulomb barrier (Gamow factor),

$$\sigma = \frac{S}{E} \exp(-31.3\sqrt{\mu/E})$$

where μ is the reduced mass in amu and E is the c.m. energy in keV. In the low energy limit, which clearly applies to cold fusion, $S \approx S_0$ (in keV barns) is 2.5×10^{-4} for pd,[5] 1.08×10^{2} for dd (including both channels),[6] and 1.15×10^{4} for dt.[7] The S function is considerably larger for dt than for dd because dt fusion is dominated by a strong ^{5}He resonant intermediate state, and it is quite small for pd since pd fusion normally occurs radiatively. Nevertheless, the penetrability factor always becomes dominant at sufficiently low energies. For *bare* nuclei the dt fusion rate exceeds the dd fusion rate at c.m. collision energies *above* ~400 eV. Likewise we note that the pd fusion rate exceeds the dd fusion rate *below* ~200 eV.

Now let us examine the implications of this factorisation for cold fusion, where some of the Coulomb repulsion is shielded by the negative electron or muon charge. It should be noted that it is the width, not the height, of the barrier that is most important and that is reduced by the shielding (the cost of a tunnel depends more on its length than the height of rock above it). This shielding eliminates the barrier up to an energy roughly equal to $14/R_0$ eV where R_0 is the classical turning point in Å. (An accurate calculation of the penetrability requires knowledge of the potential energy curve, which at this time is completely unknown for electrochemical cold fusion.) For ddμ or dtμ, $R_0 \approx 4\times10^{-3}$ Å so clearly dt fusion is the faster. For the normal molecules, $R_0 \approx 0.7$ Å and the DT fusion rate will be much slower than the D_2 rate (though both are too slow to be observed). If the classical turning distance is shortened by providing excitation energy, shortening the bond length, *or* just making the potential well shallower, then dt fusion will become relatively faster, approaching the ratio 36 for the muonic molecules (with $J=0$).[8] For the shielding distance $R_0 \sim 0.3$ Å being speculated on for cold fusion in metal crystals, the pd rate should be greater than the dd rate and this mixture should be investigated experimentally; however, the use of tritium would appear to offer no advantage.

The most dramatic effect of shielding on fusion rates previously seen is certainly muon-catalysed fusion. Nonetheless, the effect of electronic shielding has been seen in ordinary scattering experiments, most notably in the reaction ^{3}He(d,p)^{4}He.[9]

For those who would have cosmic ray muons causing the fusion in electrolytic cells, 0.1/gm/hr is a reasonable upper limit for the stopped μ^- rate in Utah basements.[10] *Conventionally*, each element would stop a fraction of the μ^- approximately proportional to its number density *and* its nuclear charge Z;[11] i.e., most of the muons would be stopped by the high-Z elements (Pd or Ti). Moreover, any dμ would initially have about an eV of energy[12] and in their thermalisation most muons would be transferred to the high-Z atoms[11] (in any accelerator μCF experiment, high-Z contaminants should be kept well below 10^{-4}). Furthermore, in liquid deuterium the ddμ molecular formation rate is comparable to the muon lifetime so few fusions are possible. *For sake of argument*, let us assume these limitations are removed by perfect channeling of the μ^- in the crystal and *extremely high deuteron densities* (so the process is not limited by collision rates). Then the *rate* would be limited by just the fusion process, and a μ^- could catalyse ~1000 dd fusions during its 2 μs lifetime *if* it were not for the 8% probability of the muon sticking to ^{3}He after dd fusion.[13] After sticking, the μ^- may be regenerated by a collision with a deuteron, but even with the assumption of infinite deuteron density the stripping probability of the μ^- from the ^{3}Heμ as it thermalises is only 32%;[14] hence under the best of circumstances (which are not very plausible), only ~20 dd fusions could be catalysed by a single muon. With the above μ^- stopping rate and an electrode mass of 3 gm, the neutron production rate would then be $\sim 2 \times 10^{-3}$ fusions/s, which falls far short of even the rate, 0.41 fusions/s, observed by Jones *et al.*[1] For dt, ~340 fusions could be catalysed by a single muon under the same generous assumptions. We note that if the Heμ collides with the high–Z element, the situation can only be worse since stripping, though highly probable, consists almost entirely of muon transfer (hence loss) instead of ionisation.[15]

If the mass M is reduced from m_μ then the fusion rate falls rapidly, but of course, one no longer has to contend with the short lifetime of μ^-. The results of Van Siclen and Jones[16] show that the bonding distance may have to be decreased by as little as a factor of 2 to obtain the neutron production rate observed by Jones *et al.*[1] Such a shrinking would be obtained for $M \approx 2m_e$. Interestingly, for transition metal crystals like Pd the effective masses m^* of the charge carriers tend to be large. So the concept of a heavy "electron" would explain the fusion of Jones *et al.* *if* the objection to the non–locality of such an entity could be overcome.

With regard to the apparently *aneutronic* fusion of Fleischmann and Pons, the equality of rates for (1a) and (1b) given by charge symmetry of nuclear forces is slightly altered by Coulomb isospin mixing. For bare nuclei (plasma fusion) the p–t channel is very slightly favoured, but for muon–catalysed fusion the n–^{3}He channel is favoured by a ratio of 1.4.[17] The latter is a consequence of fusion taking place in a $J=1$ state. In any event, it would seem quite difficult to suppress the neutron channel or to hide the tritium from assay. The normally rare radiative channel, dd$\rightarrow\alpha+\gamma$(20 MeV), would also show up in the lower energy γ detector from pair production. On the other hand, only heat would be detected if it were somehow possible for the recoil momentum to go *directly* into the palladium crystal lattice ("Mössbauer fusion"7), but such a process might be expected to be strongly suppressed by the vastly different scales. Curiously, an excess amount of ^{4}He did show up in the assay done by Fleischmann and Pons, though once again not nearly enough to explain the heating.

The above difficulties in interpreting the Fleischmann and Pons experiment in terms of dd fusion leads one to consider the possibility of other nuclear reactions that might not produce neutrons or tritons. Since the electrolyte contained lithium then ^{6}Li(d,α)α fusion is possible at the surface or with long lithium drift times into the palladium; ^{8}Be has a rich set of resonant energy levels, but it would be difficult to suppress ^{6}Li(d,n)^{7}Be and ^{7}Li(d,n)$\alpha\alpha$. Another difficulty is the smaller penetrability that comes from the greater nuclear charge, the higher reduced mass, and the larger bond distance. For similar reasons, a cold fusion reaction of d with Pd can be considered highly unlikely. Here again we note that there is the possibility of reaction with p as well as d, namely ^{7}Li(p,α)α.

In conclusion, a great deal of insight into electronic cold fusion is gained by the analogy to muon–catalysed fusion, which has been carefully studied for many years. However, we can state with high confidence that the recent electrochemically induced results can not be due to cosmic ray muons. In all this discussion, we have assumed that the current observations[1,2] truly belong to the cold fusion family. We might point out that they could be but a distant cousin. For example, it is known that dd fusion neutron emission can accompany the fracture of a LiD crystal;[18] the interpretation is that deuterons are accelerated to keV energies by the strong electric fields at the propagating crack. Such an effect might occur in the embrittled Pd or Ti crystal, in which case we would have microscopically *hot* fusion under very unusual conditions.

Acknowledgements

To many people at Birmingham, Durham, LANL(especially G. M. Hale), RAL, UKAEA, Oxford (including J. D. Jackson), and UCLA(C.–Y. Hu), but we do not hold them responsible; to C. O. Blyth who suggested collecting this information together.

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==== Top of file ====

For Dr. Roland Pease, NEWS AND VIEWS.
AND Dr. Laura Garwin, Physical Sciences Editor, Nature.
FAX 011-441- 836-9934

April 16, 1989 23:27 New York time.

Dear Drs. Pease and Garwin:

This is written on the assumption that I have 1200 words because the actual paper(s) will be published in the following issue. However, I have not actually counted the words, and I apologize for sending this precursor without being able to format it visually for FAX transmission. It will be double spaced. By 0830 EDT, I will send the formatted version, but you may want to work on this sooner and to send me lined-out and marked up copy to my FAX at IBM:
for Richard L. Garwin (914) 945-2141.

If I have only 900 words, please hint or direct modifications. Let me just explain my whereabouts on Monday. I will be at Yorktown, at an IBM Science Advisory Committee meeting all day. However, I am very much available, both to read and to write. The easiest way to communicate with me is by FAX, but

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Advisory Committee meeting all day. However, I am very much available, both to read and to write. The easiest way to communicate with me is by FAX, but
if you want to talk with me, call me at my regular number (914) 945-2555 and ask for me to return the call within a few minutes.

After my meeting starts at 0830, I may find a FAX closer to the conference room. If so, I'll let you know. Also, I hope to find first thing a FAX with the names and addresses of those at the Erice Forum, so that I can clarify some "??" in the Meeting Report.

Sorry to make you puzzle out superscripts: &sup.xx&sub. and subscripts, etc.

R.L. Garwin
****

An International Forum on Cold Nuclear Fusion*

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* April 12, 1989, at "Ettore Majorana" Center for Scientific Culture, Erice, Sicily, convened by Antonino Zichichi, Director.

---

began with presentations by M. Fleischmann&s1.

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gas cells, but some, at times, in metal hydrides.

"SOMEBODY IS GOING TO EAT HIS HAT" (Maiani, ??)
"WE ARE ALSO HUMAN, AND NEED MIRACLES, AND HOPE THEY EXIST" (Ponomarev, USSR).

A few neutrons each second (or a few thousand) from an electrolytic cell may be cold nuclear fusion, or may be due to an "arcs and sparks" origin. Within the next few weeks, experiments will surely show whether cold nuclear fusion is taking place; if so, it will teach us much besides humility and may indeed

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provide insight into significant geophysical puzzles&s2..  Large heat release
from fusion at room temperature&s1. would be a multi-dimensional revolution;
I bet against its confirmation.
mmmm

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  11:55 pm                             Sunday April 16, 1989
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For Qiu Zhaoming, FAX: 011-86-1-256-1268
or Telex: 22040 BAOAS CN

April 16, 1989
Dear Qiu:

This is a draft report. Since you sat at the meeting next to me, I would be grateful for any corrections. I hope this is helpful to you and apologize that you will have to puzzle out the abbreviations, since this is sent without having been formatted pictorially.

Richard L. Garwin (Reply to FAX in USA (914) 945-2141.

An International Forum on Cold Nuclear Fusion*

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gas cells, but some, at times, in metal hydrides.

"SOMEBODY IS GOING TO EAT HIS HAT" (Maiani, ??)
"WE ARE ALSO HUMAN, AND NEED MIRACLES, AND HOPE THEY EXIST" (Ponomarev, USSR).

A few neutrons each second (or a few thousand) from an electrolytic cell may be cold nuclear fusion, or may be due to an "arcs and sparks" origin. Within the next few weeks, experiments will surely show whether cold nuclear fusion is taking place; if so, it will teach us much besides humility and may indeed provide insight into significant geophysical puzzles&s2.. Large heat release from fusion at room temperature&s1. would be a multi-dimensional revolution; I bet against its confirmation.
mmmm

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Date: Fri, 14 Apr 89 15:56:10 PDT
From: muller@ux1.lbl.gov
To: rlg2@ibm.com
Subject: yes I got your mail

Yes I got your mail, and found your comments on Chapline enlightening.
Please send me your paper as soon as you have it. Has Chapline
responded to your criticism yet?
Incidentally, I have received my IDA clearance.
Best regards.
Rich

Originally-From: rrh@june.cs.washington.edu (Robert R. Henry)
Newsgroups: sci.physics,sci.chem,alt.fusion
Subject: UWash Observes Tritium Production in FP Cell
Date: 13 Apr 89 18:22:29 GMT
Organization: U of Washington, Computer Science, Seattle

I attended a press conference at 0930PDT Apr 13, 1989 on the University of Washington Campus. What follows is my retyping of the press release. My brief comments are in square brackets. I am a computer scientist, and not involved with any of this work.

April 13 1989

UW students detect signs suggesting 'cold fusion'

An experiment conducted by two graduate students in physics at the University of Washington may be producing indications of a fusion reaction at room temperature..

The experiment, conducted by Van L. Eden and Wei Liu, involves the use of a test tube containing "heavy water" (D20) in which a palladium electrode and a gold wire [they didn't have any platinum] are placed. The electrode and wire are connected to a power source [█10v, █1ma]. The palladium electrode, which is actually a hollow rod with one end sealed, is connected to an ultra high vacuum chamber which serves as part of a detection system.

After about 3.5 hours, a [quadrupole] mass spectrometer connected to the vacuum chamber began to detect a mass component [mass 5] that is a possible indicator of tritium, a radioactive isotope of hydrogen that is commonly produced in fusion reactions. Their apparatus continued to detect this mass for 10 hours.

As a control, they shut down the apparatus, filled their test tube with ordinary water and ran the experiment again. The signal that was suspected to be tritium dropped to below measurable levels, and was not detected again.

Finally, Eden and Liu ran the experiment a third time with heavywater and observed the return of the signal, showing that if indeed the signal is from tritium, they have found a way to start and stop the fusion process.

Eden and Liu began their work after scientists at the Univ. of Utah announced their experiments suggesting fusion could occur at room temperature. "We happened to have some heavy water in the laboratory, and I was able to get some palladium pretty easily," Eden said. They worked steadily for about a week before obtaining their results.

Normally, scientists expect a fusion reaction to produce substantial quantities of neutrons, but Eden and Liu have not detected a ny, although they are not using a particularly sensitive detector.

Eden and Liu cautioned that while their results are suggestive of a fusion reaction, they are by no means conclusive, since they have not ruled out other explanations for their observations.

Eden and Liu have not yet measured the energy output of their apparatus [hard to do in the mass spectrometer set up they are using], but Eden estimates, assuming the observed signal is tritium, that approximately as much fusion energy was released as electrical energy was added to the system.

These findings have been submitted to Physical Review Letters. Other researchers looking into the room temperature fusion claims have reported mixed success in finding indications of a fusion reaction. Eden and Liu's observation of the possible fusion reaction byproduct, Eden said, may be due to the novel palladium electrode geometry and the detection system they used.

Eden and Liu are graduate students studying surface science--the way atoms and molecules stick surfaces and move around. Both came to the UW in 1986-- Eden from England, and Liu from the People's Republic of China.

-Eden did most of the talking. 4 TV stations in attendance, plus 10 or so other kinds of reporters. Eden handled the leading questions very well, and was quite pointed about the press making a premature sensation about the fusion work. Eden repeatedly gave very cogent discussions on how science works, and how confusion and cross-fertilization are important to the process. Lets hope some of the meta lesson on the scientific process makes it into the media.

David W. Levine

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You have 1989TEL TELMSG in the WRITE MODE. Please make any changes you desire.
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Hans, I sent you without comment the paper of Steve Koonin
and Mike Nauenberg.
On very short notice (naturally, following the 03/23/89
discovery press conference of Fleischmann and Pons in
Utah, and the parallel publication by Jones,
.us et al
from Brigham Young University)
I helped organize for Nino Zichichi a one-day Workshop in
Erice.
Since it was arranged on the only day I could attend,
I was finally forced to go.
.sk
I believe there were about 40 people invited to the Workshop,
plus about 60 scientists invited to "observe,"
plus about 40 journalists.
It really turned out quite well, in my opinion.
Fleischmann and Jones were both there, and I want to talk
with you personally about some of the discussions and
prospects.
I have agreed to prepare by Sunday evening a 900-word
report on the Workshop, for publication in
.us Nature,
and the rest of this note to you is a beginning at getting
my thoughts in order for that paper.
Of course, you and I will be presenting papers Thursday
morning on Science Advising, but perhaps we can talk at
lunch or Thursday afternoon.
There are some real physics puzzles involved,
and there is one bit of information that you and I share
that I could not mention at the conference.
.sk
(Hans, this is terribly rough, but I want to have it
available by FAX so that I can leave it with you.)
.sk
About the only agreement at the International Forum on
Cold Nuclear Fusion
("Ettore Majorana" Centre for International Culture,
Erice, Sicily, 04/12/89, convened by A.^Zichichi, Director)
is that there should be ~~far~~ more reactions of the type;.br
P+D^=^He-3^+^gamma (5.4^MeV)^(i);.br (0.82 MeV)
then D+D^=^He^3^+^n ~~(q^=^???^MeV)~~^(ii);.br (2.45 MeV)
or D+D^=^H-3^+^P ~~(q^=^???^MeV)~~^(iii);.br 1.01 MeV (3.02 MeV)
in the breathtaking experiments of Jones, et al
(Jones, S.E.; ... this issue) and Fleischmann, et al
(Fleischmann, M.; Pons, S.; Hawkins, ... J.^Electroanalytical
Chemistry) which are claimed to demonstrate fusion of the
light nuclei in electrochemical cells a few centimeters
in size driven by currents of less than an ampere at a few
volts.

XYZ This version is now [illegible], wrong section
~~Jones et~~ BYU
(Brigham Young University)
(Brigham [illegible] vs U.U. (University of Utah)
is published in this issue!

BYU
UU

In both cases, the authors attribute the fusion to deuterium
driven into titanium (SEJ) or palladium (MF) metal cathodes
in a cell of heavy water to which soluble salts have been added---
a simple 0.1^M solution of LiOD in the case of MF,
and a witch's brew of salts, including Li and Pd (at pH 3), in the case
of SEJ. ~~[illegible]~~3
.sk
SEJ claims about 0.6 fusions per second in a few grams of
Ti sponge cathode, on the evidence of 170^+/-^23 excess
counts attributable to the 2.45^MeV neutrons of Eq^ii.
Some 13 8-hr runs preceding 03/23/89 are included;
only background ~~has~~ ve been ~~obtained~~ made since then. no measurements
MF claims some 4000 neutrons per second from cast and
machined Pd rods 1, 2, and 4^mm in diam and 100^mm, 100^mm,
and 12^mm long, respectively, detected by the 2.2^MeV
gamma rays created by the capture of the neutrons of
Eq.^ii on the protons in a water bath

than a microsecond).
The claimed fusion rate per deuteron λf 06 ~~is thus~~ some
lambda-F ~~of~~ 10**-23 per sec for SEJ and ~~lambda-F of~~
10**-20 for MF, contrast~~ing~~s with a calculated
(and undetectable) lambda-F of 10**-~~74~~ 6 for the much
higher local deuteron density in the deuterium molecule.
The repulsive Coulomb barrier Z1^Z2 ee/r of 0.6^MeV
for Z1^=^Z2^=^1 accounts for the very small anticipated
rate, non^zero because of quantum mechanical tunneling
by the particles with a barrier penetration factor B^=^~~e** (....)~~. $e^{-\int_0^{r_0} k(r)\,dr}$
The barrier penetration calculation is almost identical
with that involved in decay of heavy nuclei by alpha
particle emission;
and for specificity has been carried out for ~~2d~~ two hydrogen isotopes bound in
a diatomic molecule by "electrons" of mass M* instead of Me.
Thus, lambda-F can be calculated (Koonin, CalTech) as

with $k(r) = [2\mu(V(r)-E)]^{1/2}$
~~μ is the reduced mass~~
~~of the~~ [illegible] r0 is the
"classical turning point"
at which the repulsive potential
V(r0) equals the
kinetic energy E.

.sk;.fo off
.tp 13 23 33 43 53

M*/Me	1	2	5	10	200
lambda-~~DD~~ d-d	-63.5	-40.4	-19.8	-09.1	+6(**)
lambda-~~Pd~~ p-d	-55.0	-36.0	-19.0	-10.4	??

.sk;.fo
where the entries in the table are the base-10 logarithms
of the fusion rate per deuteron.
.sk
Other groups agreed with these calculations which use a
more accurate molecular potential and correct some errors
in earlier calculations.
.sk
Even though ~~Eq~~ (i) is 10**6 times slower than ~~Eq~~ (ii) for the
tiny fraction of particles within the range of nuclear
forces, the coefficient of the square of the wave function
and the quantum mechanical ... calculations, D depends so
strongly on the reduced mass (M1^x^M2)/(M1^+^M2) of the
nuclei that the ~~P-D~~ p-d rate should be much faster than the
d-d ~~D-D~~ rate. Even if heavy electrons should be able to screen the Coulomb force as for M*/Me = 10, λ_{p-d} would be faster than λ_{d-d}. μ ≡
But the 5.4^MeV gamma is not seen by either group,
casting grave doubt on the occurrence of the D-D reaction, itself.
Although protons are present only to about 0.5% in the heavy
water, there is a separation factor of some 9.5 (Fleischmann)
expected in the electrolysis, so about 5% of the sites in
the metal should be occupied by P rather than^D. [Koonin suggests ~~that~~ cold fusion (if it exists) ~~[illegible]~~ is more likely p-d? [illegible] ~~[illegible]~~
.sk
Spokesmen for 3 experimental groups (M.M.^Broer, AT^&^T Bell Labs;
J.F.^Ziegler, IBM; R.^Hahn, Brookhaven) reported no detection
of neutrons or gammas, with Broer stating specifically that
a 1^mm by 100^mm rod had been run to 500^mA per square
centimeters for 10 days, with no neutrons detected above
a source limit of 1^per sec, compared with the 1000 per sec
claimed by Fleischmann.
Ziegler reported that there were no fusions of type ii or iii
detected at an upper limit of 10**-3 per sec per cubic centimeter---
a factor 10**3 below SEJ and 10**6 below MF.

— the steady release
of heat, if true,
can't be explained by chemical
or electrochemical forces.

(Garwin, IBM)

.sk
Neutron~~s~~ counts were indeed reported by Celani "for the first few minutes
and then dropping off to background after 20 minutes",
but no data was shown.

probably The cells ~~[illegible]~~ in
~~[illegible]~~ caused by the electrochemical
impurities of [illegible] explosive
(D occupying sites in the
pd crystal of [illegible] eV
in [illegible] the Pd.

.sk
The astounding claim of ~~Fleischmann~~ MF, however, is to obtain
a ~~the~~ steady evolution of
.us heat
of some 10 watts per cubic centimeter for 100 hours or more---
some 4^MJ/cc or 600^eV per atom, and an explosive ~~[illegible]~~ release of heat that melted and partially vaporized
Since 1^MeV is 1.6^x^10**-13 joule, some 6^x^10**12 fusions
per S^x^cc (each giving 10^MeV) would be needed to produce

— none above a
base energy) which energy is
likely to be released explosively when it
cannot be suitably let off, but

this heat.
The neutrons or gamma rays from i or ii would not only be
10**9 times more numerous than those detected;
they would have given a lethal dose to the experimenters
in less than an hour at a distance of 1^meter.
.sk
Except for MF, none at the Workshop had seen such heat,
nor should they have, if the ratio of heat to neutrons ~~is~~ were
the same in the various experiments.
No special surface preparation of the Johnson-Mathey Pd
was used (MF), they were cast and machined, and their
surface "wiped with filter paper--
.us not
kleenex."
An 8-mm diam rod "did not work."
The lithium was "mostly Li-7."
Some lithium "went into the Pd lattice, but Fleischmann
"does not wish to report any analysis of cathodes after
experiment."
"It is normal to have blackening of the cathode,
but the thermal yield remains the same."
"Litium in the electrolyte is important to prevent
cracking of the cathode."
In reply to comments that a critical experiment would be
a run with ordinary water, Fleischmann replied firmly
"I am not prepared to discuss light water and will not
be for some months."
.sk
Turning from how one could realize the ~~fusion rate~~ λ_f [illegible] of
10**-10 per particle per second required to account for
the heat, Denys Wilkinson responded to his assignment
to identify ~~such~~ a reaction that would produce no neutrons or gamma
He suggested that one might under these conditions have
the D-D reaction totally dominated by the Coulomb
force, and not just the barrier penetration.
If the initial state were not a state of definite
isotopic spin (isospin), as is the deuteron normally
(T^=^0), then the two component isospin states would
cancel in the reaction going to neutron and would
reinforce in the branch going to the proton.
It was pointed out (Garwin) that this was ~~not~~ an
additional channel, ~~but only a selection of one channel~~
already involved, ~~and that one would pay dearly in~~
~~projecting the normal deuteron state onto this isospin~~
~~(T^=^1) state.~~
~~Furthermore, the gain in reaction~~
~~.us rate~~
~~would be very modest, and can be calculated as the~~
~~thinning of the barrier by the size of the deuteron~~
~~wave function (including the tail), and also a thinning~~
~~corresponding to the fact that the protons would not need~~
~~to approach one another so closely.~~
~~An estimate of such effects amounts to about 25% increase~~
~~in rate for the non-definite isospin state,~~
but one would pay at least a factor 300 to achieve this state.
.sk
Actually, the reaction D^+^Li-6 would provide a radiationless
energy yield, and if D^+^Li^6 were to occur, Dp^+^Li-7
would go much faster in the lattice (Garwin).
The Coulomb barrier for these reactions is three times as
high as for Z1^=^Z2^=^1, but so long as one is imagining
screening the Coulomb potential to the required extent,
one could screen it to a factor 1.73 times smaller distance,
where the ~~barrier~~ penetration probability of the higher barrier would be
the same as for D-D.
Not quite the same, because the reduced mass of P is greater

([illegible])

Pr[illegible] — dynamic effects in which the deuterons orbit with the same speed as electron equilibrate with the same equilibrium state.

~~[illegible]~~ per 10^9

Oh no

$\chi^2 = 1/670$

2 degrees of freedom,

$\chi^2 = 4$ gamma,
10^{-4}

the T=0 channel
not a suppression

; ~~an estimate of the relative [illegible]~~
~~the fact~~ the crude calculation shows only a few % ~~addition~~ about to the fusion rate.

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That's all I have time for right now.
I will add to this and clean it up, Hans, but I thought
you would be interested in seeing this report even in its
rough state.
.FO OFF;.SK
Sincerely yours,
.SK 3
Richard L. Garwin
.CM;Forwarded in his absence
.SK
.TR $ 41;.TR + 41
.IN 0;.SK;.FO
.TR $ $;.TR + +;.PX > OFF;.PX - OFF
RLG:jtml:104%HAB:041489.HAB
.CM 735 - No. pages: xx4 Code: ol Desc.: I sent you without comment the paper of
 Koonin and Nauenberg. I helped organ
.CM 768 - This file is ready to be logged.
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I believe it premature to quote "Don Giovanni" -- Act III "in Cts

altra cena, dal [illegible] ?"

Non l'avrei giammai creduto...

ma farò quel che potrò..."

(I, ~~seven~~ would have never believed that, but I'll see what I can do with it.)

Date: Friday, 14 April 1989 1119-EST
From: SHERMAN@PENNDRLS
Subject:
To: RLG2@YKTVMV

Dear Dick,

Thanks for your comments on the security chapter, which just arrived.

Mush Goldberger told me last night that you were studying the cold fusion papers, claims etc. and that you had met in Italy with the some of the experimenters.

If you write anything on this could you bitmail it here?

Roy Middleton at our tandem had titanium foils which he loaded with D2 and H2 twelve years ago. He is the world's expert on using tandems for mass spectometer measurements. Unfortunately they ran tritium some years ago so there is contamination. He saw a difference but he knows how tricky these experiments are. I believe he is sending samples to Harry Gove who does not have the background problem.
Roy is at 898-8832.

Sherman